

Prof. Hoffmann, of Berlin, whether that doctrine any longer finds support among scientific men in Germany. *His reply was a most emphatic negative*; the doctrine, he said, being one which no man of science with whom he is acquainted would think worthy of the slightest attention. Yet in Mr. Wallace's judgment (*query* in Mr. Crookes's also?) the unanimous verdict of the scientific world of Germany, to say nothing of England, is a prejudiced one; only Mr. W. and his spiritualistic allies appreciating correctly the real force of the evidence originally advanced by Reichenbach, and confirmed by those trustworthy (?) authorities, Drs. Ashburner and Gregory.

In thus setting his own judgment on a question which lies altogether outside the scientific domain which he has made his own, against the unanimous verdict of the eminent physicists and physiologists who have carefully "tried" the Od-force and "found it wanting," and in rebuking myself and those who think with me for our incredulity, does not Mr. Wallace put himself somewhat in the attitude of his old opponent, John Hampden, who thinks everybody either a fool or a knave who maintains the earth to be round?

WILLIAM B. CARPENTER

October 22

Potential Energy

WITH reference to the views of "John O'Toole" on the subject of energy perhaps you will allow me to say how one of the class to which "poor Publius" belongs has conceived the matter of terminology with satisfaction to himself.

1. Energy being unanimously defined by "the doctors" to be "capacity for doing work," and also energy conveying in its derivation the notion of activity, this term is properly applicable only to the bodies of material systems the motions of which are contemplated. Hence all energy is in its nature kinetic—the very term kinetic is logically included in the term energy.

2. When a material system is in motion it actually possesses, *ipso facto*, a capability of doing work, that is to say, it has actual energy.

3. When in any configuration of the system we contemplate as possible the action of causes which will alter its motions and give it a second configuration, the excess of the energy which it would possess in this second configuration over the energy which it possesses in the first is properly called its potential energy in the first configuration.

4. The assertion that in any configuration the sum of the energies, actual and potential, of a material system is constant, is what Kant would call an analytical proposition, or what "X." (quoting Herschel) calls "only a truism after all." But I further remark—

5. That this truism is not the principle of the conservation of energy, but that this principle is a true "synthetical proposition" which some fairly regard as an almost immediate deduction from Newton's third law, and which others regard as proved by often repeated and much varied experiment; and hence that "X.'s" statement of this great principle in the form—"The sum of the actual and potential energies of *the universe* is a constant quantity," (the italics are mine) is not its proper definition.

6. That, leaving the consideration of bodies, and referring to forces, the term to be employed instead of energy is work, and that the term analogous to the "potential energy of bodies" is the "potential work of forces," this latter being the amount of work which they are capable of doing in displacing their points of application from their actual configuration to any fixed chosen one.

7. That by the expenditure of a fixed amount of work on any material system the same amount of actual energy (whose type is $\frac{1}{2}mv^2$) is under all circumstances produced, and that, through whatever forms this actual energy is made to pass, if the whole of it is always utilised, it will finally be reconvertible into the same original amount of work, this being the principle of the conservation of energy.

8. That instead of the statement in 5, we must substitute the synthetical proposition that "the sum of the actual energy of the bodies in the universe and the *potential work of its internal forces* is a constant quantity," and the same is true of every material system which is regarded as complete in itself; or in other words, wherever and however a given quantity of potential work is lost by the forces of the system, this always appears in the shape of a fixed quantity of actual energy, in the form which we call heat, or in some other.

Hence we have energy, actual and potential, of bodies; and work, actual and potential, of forces.

A few remarks in conclusion. "J. M." has very happily illustrated the propriety of the expression potential energy, as, in strict consequence of the definition of energy, a potential capacity of doing work; and if in his illustration the "power of purchasing" is considered with reference to a further object, there may be not merely a "double remotion from" what we may regard as "tangibility," but a remotion of a higher multiple order. "W. G." has well explained that it is only in consequence of the fixedness of the earth that the potential energy of the system of the earth and stone is by the "doctors" located in the stone. Finally, I can hardly conceive how "X.," who has devoted so much attention to the literature of this subject, can have fallen into such a grievous error with regard to the clock.

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G. M. MINCHIN

YOUR "Potential Energy" correspondents will find three letters on the "Conservation of Energy" in the *Engineer* for January 12 and 19 and February 2 which may interest them. The writer "ΦΠ" assumes that all the phenomena of force are explained by the theory that only matter and motion exist, and that what we call potential "energy" is only "quantity and motion," which motion is indestructible but diffusible. Z.

London, October 20

Origin of Contagious Diseases

I HAVE been much struck by the following passage in Dr. Richardson's address, *NATURE* (vol. xvi. p. 481):—

"(c) That as regards the organic poisons themselves and their physical properties, the great type of them all is represented by the poison of any venomous snake. . . . It is the type of all the poisons which produce disease."

Now has it been really proved, by experiment, that the poison of snakes produces the effects characterising the contagia? viz.,

"(d) . . . Each particle of any of these poisons brought into contact either with the blood of the living animal or with certain secretions of the living animal, possesses the property of turning the albuminous part of that same blood or that same secretion into substance like itself. . . ."

In other words, if an animal is suffering from snake poison does its blood or any of its secretions acquire the power of transmitting the disease, *i.e.*, the effects of a snake's bite, to another individual, as is the case with an animal affected with carbuncle, glanders, hydrophobia, &c., &c.?

Unless this question has been decided in the affirmative it would appear rather difficult to uphold the sentence (c) as quoted above.

D. W.

Freiburg in Brisgau, G. J., October 14

[Dr. Richardson informs us that D. W. does not properly understand his argument. Dr. Richardson does not suppose that the person or animal poisoned from a poisonous snake is, in turn, poisonous, although that may be the fact. He merely uses the illustration that as a poisonous snake secretes a poison so an infectious person is for the time secreting a poison.]

I SEE by your issue of October 4, that Dr. Richardson has honoured me by mentioning my name and placing me as the first, in modern times, to advocate the hypothesis that living germs are the exciting agents of epidemic and infectious diseases. But he says further, "I protest, I say, that this hypothesis is the wildest, the most innocent, the most distant from the phenomena it attempts to explain, that ever entered the mind of man to conceive." It may be so, but I look in vain through the whole story he narrates in his lecture to find a rational substitute for it, and it appears to me desirable at the present juncture that the principles of the germ theory, as I have interpreted them, should stand side by side with Dr. Richardson's "glandular theory." It is now nearly thirty years since I endeavoured to find some common root or cause for those diseases which we find in plants, animals, and man, and which are communicable among the individuals of each order in nature; also, in some instances, from one order to another. During that thirty years every step in scientific research and medical experience as far as my inquiries have carried me, has tended to confirm the views I put forward in my original "Essay" and in subsequent papers read before the Epidemiological Society. Notably the latest advocates of a germ theory are two of our most eminent men, the one a leader in science, the other a leading physician. I need hardly say I allude to Prof. Tyndall and Sir Thomas Watson; surely these

gentlemen cannot be charged with committing themselves to an hypothesis "the most distant from the phenomena it attempts to explain."

Now if it can be shown that the germs of disease are subject to the same laws as other living things and exhibit similar phenomena, and further, that without the inference that they are endowed with vital properties, it is impossible to unravel the most striking character which they present to us for consideration, viz., the fact that they reproduce their kind, then I think there is more reason for following up, in all its intricacies, the germ theory, than to start with an assumed catalysis, molecular motion, and a glandular matrix, as suggested by Dr. Richardson.

Starting, then, from the indisputable fact that the *materias morbi* of every communicable disease reproduces its kind, I have considered this a primary law, and have tabulated other laws which are associated with living beings by which it will, I think, be found that there is a parallelism of a kind to attract and rivet attention, especially, too, when many otherwise inexplicable circumstances bend to this hypothesis.

Primary Law of Reproduction, by which all living things reproduce their kind.

SECONDARY LAWS.

Objective Laws.

1. The diffusion or dispersion of germs.
2. Their static existence.
3. Limited duration of active existence.
4. Period of development, maturity, and decay.
5. Intermittent reproduction.

Subjective Laws.

1. Seasons of activity.
2. Climatic influence.
3. Relation to latitude.
4. Subjection to 'physical forces.'
5. Influence of locality.

Without amplifying this subject, which would carry me far beyond the limits of an ordinary communication, I will only add that though the above tabulation is very imperfect, there is quite sufficient for any one who will follow out the ideas conveyed by it to trace the intimate relation that exists between living beings and the germs of disease. I would refer finally to the fact that many diseases in men and animals have yielded up living germs as their cause, chiefly, I may add, skin diseases it is true; but *aphtha*,¹ closely associated with diphtheria, is, I think, acknowledged by all unprejudiced persons to have its origin in an unmistakable and demonstrable germ.

JOHN GROVE

The Zoological Relations of Madagascar and Africa

WITHOUT entering into the details of this very difficult question I wish to be allowed to state some of the general reasons which have led me to a different conclusion from Dr. Hartlaub,² and also to point out where he has not quoted my opinions with perfect accuracy. Instead of saying that "the fauna of Madagascar is manifestly of African origin," my actual statement is as follows:—"We have the extraordinary fauna of Madagascar to account for, with its evident main derivation from Africa, yet wanting all the larger and higher African forms; its resemblances to Malaya and to South America; and its wonderful assemblage of altogether peculiar types" ("Geog. Dist. of Animals," vol. i. p. 286). My reasons for believing in the "main derivation" of the fauna from Africa can only be understood by considering the theory, now generally admitted, of the origin of the fauna of Africa itself. All the higher mammalia are believed to have entered it from the northern continent during the middle or latter part of the tertiary period, and the occurrence of *Psittacus* and of forms supposed to be allied to plantain-eaters and to *Leptosomus* in the miocene of France, render it probable that many of the peculiar groups of African birds had their origin in the old Palearctic region. Now Madagascar presents many cases of special affinity with South Africa, especially in insects, land-shells, and plants; and if we suppose it to have formed part of a South African land before the irruption of the higher mammals and birds from the north, we shall I think account for many of its peculiarities. Such facts as its possessing *Potamocharus* and the recently extinct *Hippopotamus*, while it has thirteen or fourteen peculiarly African genera of birds against four or five that are peculiarly Oriental; of its having many African genera of lizards and tortoises; of its butterflies being decidedly African; of its numerous African genera of Carabidæ, Lucanidæ, and Lamiidæ; while the specially Oriental affinities of its mammals, reptiles,

and insects are hardly if at all more pronounced than the South American affinities of the same groups,—all seem to me to warrant the general conclusion that the "main derivation" of the Madagascar fauna is from Africa.

Dr. Hartlaub speaks of my "attempted parallel between Madagascar and Africa, and the Antilles and South America" in such a way that his readers must think I had dwelt upon this parallel in some detail as being special and peculiar. The fact is, however, that I have always referred to it in a very general way. At p. 75 vol. i. I say: "The peculiarities it (the Malagasy sub-region) exhibits, beings of exactly the same kind as those presented by the Antilles, by New Zealand, and even by Celebes and Ceylon, but in a much greater degree." And again, at p. 272, vol. i., I speak of it as "bearing a similar relation to Africa as the Antilles to Tropical America, or New Zealand to Australia, but possessing a much richer fauna than either of these, and in some respects a more remarkable one even than New Zealand." This general comparison with the two other great insular sub-regions is, I think, justifiable, notwithstanding great differences of detail. There is in all a rich and highly peculiar fauna, a great poverty of mammalia, and a total absence of many large families of birds characterising the adjacent continent, together with special points of resemblance to distant continents or to remote geological periods.

It seems to me that such a problem as this cannot well be solved by means of a group which, like birds, do not require an actual land-connection in order to reach a given country; and, if all land animals are taken into account, the evidence does not appear to warrant the supposition of a recent land-connection of Madagascar with India or Malaya. At a very remote epoch such a connection may have taken place, but if we are to give any weight to the general facts of distribution as opposed to those presented by birds only, the union of Madagascar with South Africa is more recent and has had more influence on the character of the Malagasy fauna. The numerous and very remarkable points of affinity between Madagascar and South America in almost every group except birds, are not alluded to by Dr. Hartlaub, yet they would equally well support the notion of a former union of those two countries independently of Africa. It seems, however, more consonant with our general knowledge of distribution to consider these as cases of survival of ancient and once wide-spread types in suitable areas; and this is a principle that must never be lost sight of in attempting to solve the problems presented by such anomalous countries as Madagascar.

ALFRED R. WALLACE

Selective Discrimination in Insects

YOUR correspondent S.B., in his letter NATURE of yesterday's date, must be referring to some short abstract only of my lecture on flowers and insects. I quite agree with him that odour is very important in attracting insects, and dwell upon it in my lecture, as well as in my little book on "Flowers and Insects." A striking illustration is afforded by night flowers, which often become peculiarly odoriferous towards evening, as has been already pointed out by various observers.

S.B. attributes, I think, too little importance to the colouring of flowers, but his letter shows him to be a careful observer, and I hope he will continue to devote his attention to the subject.

He would find H. Müller's "Blumen und Insekten" a mine of most interesting and accurate observation.

London, October 19

JOHN LURBOCK

Protective Colouring in Birds

WITH reference to the statement in my "Naturalist in Nicaragua," p. 196, that the macaw "fears no foe," &c., the well-known geologist, Prof. Gabb, sends me the following information:—"I willingly comply with your request to repeat the statement about the *Kukong pung* or macaw hawk of Costa Rica. Not having your book by me now I cannot refer to page nor quote your statement exactly. But as I recall it, you speak of the great red and blue macaw as being so well defended as to need no protective colouring, and that no hawk dares attack it. In this you are mistaken. Not only have I seen on several occasions heaps of the unmistakable feathers of the bird in the woods, left in the manner that all woodsmen recognise as hawk's work, but I have the statements of various Indians, not in collusion, confirming each other, and finally I have had the bird pointed out to me (I am not sure but that it may occur in the collection I sent to the Smithsonian). It is a fair-sized hawk of dark

¹ See *Medical Times*, 1851, vol. ii. p. 95.

² NATURE, vol. xvi. p. 498, and the *Ibis* for July, 1847, p. 334.